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PSYCHOLOGY AND SCIENTIFIC METHODS

All communications to the Editors should be addressed to PROFESSOR FREDERICK J. E. WOODBRIDGE, or DR. WENDELL T. BUSH, Columbia University, New York City.

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THE JOURNAL OF PHILOSOPHY
PSYCHOLOGY AND SCIENTIFIC METHODS

ILLUSIONS OF DIRECTION ORIENTATION[1]

THE consciousness of the cardinal points of direction is a subject that has not received the attention which its practical bearings in orientation justify. Mr. Townbridge has pointed out that two fundamental methods of orientation are employed.[2] The method

[1] This paper was read before a number of my colleagues in the department of philosophy and psychology. In the discussion which resulted it became evident that some of the experiences and difficulties here described are foreign to individuals who were brought up in childhood in cities. These individuals acquired the habit of orienting themselves with respect to certain conspicuous buildings and streets and thought very little of the cardinal points of direction. One reports that the terms east, west, north, and south had a much more particular meaning to him than they did to me. To him as a child the east side of the city (Chicago) was merely the side on which the lake was located; west meant the other side. To such individuals, it appears, the names of the cardinal points have been associated with domi-centric orientation in a purely practical way, and this habit has become so fixed that the persons in adult life on going into a strange city at once establish similar practical associations with very little reference to the sun, or to the cardinal points of direction. Their orientation seems to remain largely domi-centric, to use the term in a wide enough sense to include the sort of practical adjustment just described. My own early experiences were in a country, on a farm, where mountains were constantly visible even to distances of over fifty miles. The practise was to determine the time of day, especially noon, by the position of the sun. The north star early became a familiar friend and guide in the night time. In hunting-trips extensive cross-country excursions were frequently taken both in the daytime and in the evenings when stars were the safest guides as to direction. It is possible, therefore, that the cardinal points of direction have become much more important guides to me in later orientation in cities than to many of my readers. From our discussion it appeared that individual differences of this kind are largely due to early training. If this proves to be the case will it not make even more emphatic the need of attention to orientation in early geography teaching? For more consistency in one's larger geographical conceptions, and for the purpose of preserving a broader intellectual interest in the geographical and astronomical aspects of nature, is it not a good thing to emphasize the ego-centric orientation in the child's early education? Because of these individual differences this paper must be regarded as merely preliminary. It is hoped that opportunities will be afforded for a more extensive study of this subject in the near future.

[2] "On Fundamental Methods of Orientation and 'Imaginary Maps,'" *Science*, XXXVIII., 1913, pages 888 ff.

generally used by civilized man except in the immediate vicinity of his home is called the *ego-centric* method, because it is based on the intersection at the ego of the lines going out to the cardinal points of direction. The second method Mr. Townbridge calls the *domi-centric*. Orientation by this method is carried on with reference to an established central place, the "home." Birds, mammals, fishes and other animals, young children, and savages are said to employ domi-centric orientation, a method making possible considerable reflex development and the establishment of familiar paths and trails. The successful use of an ego-centric system of orientation implies considerable imagery. It gives only an indirect clue to the direction of the home or starting-place, requiring the individual, therefore, to image the general situation in the form of a map. This map, it should be noted, includes numerous points closely related by direct association. It is evident, therefore, that such a map-construction constantly involves, along with the consciousness of the points of direction, also something of the direct, or domi-centric, orientation. Ego-centric orientation is really, then, a complex of the two systems.

This fact of complexity of the system of orientation employed by adult man seems to lie at the basis of a confusion, as it appears to the writer, in the interesting paper to which reference has been made,—a confusion the clearing up of which will likely simplify the classification of types of errors in "imaginary maps" made by Mr. Townbridge. The confusion seems to be in the failure sufficiently to separate the elements of the domi-centric from those of the ego-centric behavior. Through early faulty instruction as to the direction of certain places, without any reference to the cardinal points, a child may develop a false notion of the direction of a city and yet not be confused as to what is north, south, east, or west. This notion may conceivably have a feeling of reality associated with it even though later experience may have proved it to be false. In such a case the person is not said to be "turned around"; that is, there is no general illusion as to the cardinal points of direction. The purpose of this paper is, however, not to enter critically into the classification of imaginary maps, as given by Mr. Townbridge, but to present further data on illusions of the sense of direction.

If an older child or an adult is asked at any time to indicate which direction is north he will usually be prepared to do so even though his north is not the true north. Many persons have a "feeling" that a certain direction is north when as a matter of fact they know that it is not. They operate on the basis of two ego-centric systems more or less contradictory. We might distinguish in this regard five types of individuals, or better, five types of consciousness of direction, all of which the same individual may experience under different circum-

stances. (1) Those whose consciousness of direction is correct; their north is the true north. (2) Those whose consciousness of direction is wrong and known by experience to be wrong; *e. g.*, what seems to be north may be known to be east. (3) The same as those described in the previous class except that the consciousness of direction is not known (has not yet been found out) to be wrong. This is an earlier stage of the second class. (4) A class early pointed out by Binet and called disorientation—those who are temporarily confused as to what *seems* north. This condition, of short duration, may be experienced on suddenly awaking in a strange hotel, or on a moving train. It may persist for some time if on a cloudy day one is on a train making frequent turns. (5) This type includes the animals, very young children, and probably savages, for whom no general sense of direction has developed. Orientation is domi-centric.

Normally this consciousness of direction has arisen so gradually and by so natural a process in our experiences that we are inclined to feel that it is an innate faculty, just as our moral conceptions were at one time thought to be due to an innate moral sense. It is, however, unquestionably acquired or gradually developed from data furnished by various of the senses, and is, therefore, not a sense at all in the meaning of an original faculty of the mind. It is nevertheless convenient in this paper to use the expression "sense of direction" if it is explicitly understood to be an acquired disposition. Illusions of orientation, or of the sense or consciousness of direction, are cases of disagreement between the person's perception, or general "feeling," of north and the true north.

From the results of observations of various reliable individuals it has been found that there is a tendency for the illusion of orientation to equal approximately 90 or 180 degrees.[3] The frequency of the 180-degree illusion has led certain writers to use the term "reverse" illusion of orientation. There are at least two reasons why the illusions tend to approximate one or two right angles. It is possible that illusions of but a few degrees are not as a rule noticed, as they cause very little confusion in behavior, especially when one is going across country where streets and the usual division lines are not visible. The writer has frequently noticed that under such conditions any direction within the limits of a considerable angle could be made to appear north, if attention were centered upon it with that purpose in view. Past experience with streets crossing at right angles, moreover, unconsciously emphasizes to us the cardinal points so that we habitually classify directions according to right-angle units of divergence from the north. Thus we describe objects as being nearly east, south,

[3] Binet, A., "Reverse Illusions of Orientation" (tr.), *Psychological Review*, Vol. I., 1894, pages 337–350.

west of south, southeast, always putting the emphasis on the names of the cardinal points. The very adoption of the directions north, east, south, and west as our cardinal points of direction makes it difficult for us to think in other terms, just as the custom of figuring in the decimal system of notation makes thought in any other system difficult. In ordinary thought no other system gets any consideration at all. When first one's attention is called to the possibility of any other plan of notation the thought is somewhat stupefying. Illusions that are noticed are thus unconsciously affected by our tendency to think of directions in terms of the cardinal points. If one has lived in a city the streets of which are laid out according to the cardinal points of direction, one finds in strange cities a strong tendency to see streets following other directions as if they also run north-south and east-west. Finally, illusions of direction ranging from 30 to 60 degrees are by no means wanting.

It is remarkable that in the extensive studies of perception which have been carried out the aspect of the orientation consciousness with which we are here concerned has received so little attention. This is likely because of its fundamental relation to all our spatial perceptions. Psychology has been busy largely with the study of the individual perceptive processes themselves. The various experiences in which each of us is constantly absorbed are given their significance and general setting by associations among themselves. As is true of the heavenly bodies in our firmament, the more remote experiences are likely, by virtue of their more numerous associations, to be the more permanent. Astronomy began with the observation of the more immediate and striking phenomena. In the history of psychology we have gradually been gaining perspective and have thereby more plainly seen the inadequacy of certain earlier views. In the realms of animal behavior, where the complete environment and past history of the individual are more easily observed and controlled, the question of orientation has in many cases received explicit attention, and has been the direct object of experimentation. Even here, however, the facts are relatively complex and as a consequence numerous theories of "distant orientation" have arisen. Hypotheses of a special sense of direction have not been wanting, although no one has attempted fully to show how such a sense might operate. Such an attempt seriously undertaken, even theoretically, would doubtless have had an effect here similar to that in ethics of analyzing the "moral sense" of Shaftesbury and Hutcheson.

There is good reason to believe that a general consistency in one's perceptions of direction is maintained, as is doubtless true of the continuity of self-consciousness, by a multiplicity of factors. This condition makes direct experimental tests difficult. A careful exam-

ination of a few striking cases of illusion of orientation, however, reveal in a measure some of the factors by which the general sense of orientation is maintained. In such an analysis it will be convenient to give the descriptions in a somewhat personal form.

In the summer of 1914 I experienced in Chicago an illusion of unusual vividness. In company with others I was transferring from a north-side to a south-side car. We entered our car as it was facing west just before the turn to go south. When this car started the other members of the party had become seated, but I was still standing giving our transfers to the conductor. The suddenness of the turn which the car made while my attention was diverted to the transfers and to the maintaining of my equilibrium in the standing position "turned me around" completely. When I took my seat the car appeared to be going north. I thought that we had taken a wrong car, but was assured by the other members of the party that we were going south. Of this fact I found further proof on passing certain familiar buildings, and on our arriving at the point where the car line takes a separate course from that of the elevated railway under which we had been moving. Having usually taken the elevated-railway car, I was not very well acquainted with the street along which we were now passing; moreover, as it was dark only a momentary and imperfect view of the familiar marks along the way was possible. The illusion therefore persisted. I saw streets known to lead on the east side to the lake, but which now appeared to go westward. We came finally, on Cottage Grove Avenue, to Washington Park on the right with which I was well acquainted. Not even this enabled me to correct the illusion. Immediately, however, on stepping out of the street-car on 58th Street and Cottage Grove, in the midst of numerous perfectly familiar objects and buildings I felt an "unwinding sensation" in the head, a sort of vertigo, and presto! the illusion was gone. I saw the car start its motion and continue in the direction in which it had been going—but now it was due south.

The sensation of so sudden a "change" is a peculiar one: the various present stimuli, bringing up innumerable familiar associations and attitudes suddenly force some sort of readjustment in one's general orientation attitude. It is a striking bit of evidence that we are not passive in our perceptions and interpretations, but that everything must be seen in some system of generally consistent relations. Coming with such an erroneous sense of direction into familiar surroundings, one experiences for a time a conflict of two different systems. Sometimes the change is gradual; occasionally, when, as in the present case, the conditions are favorable, it is sudden, with possibly even a feeling of being forced by the general surrounding ob-

jects and circumstances. This condition is in some respects similar to changes in personality.[4]

A second case of illusion of direction, which I have been observing for about three months, is more complicated. In certain sections of Minneapolis I am "turned around" to the extent of 90 to 100 degrees. It is not easy to determine the exact extent of the illusion, as any one will find who attempts such measurements. The extent of the illusion also varies for different parts of the city where the illusion is experienced. Though this illusion is readily dispelled when I go into certain parts of the city, or outside the city, it has been impossible for me to get rid of it in the parts of the city with which I first became acquainted. In these sections where the illusion prevails, viz., in part of south and of southeast Minneapolis, the streets are not laid off according to the cardinal points on the compass, but follow the general direction of the river. Having not been accustomed to this condition, though I have, of course, frequently met with it, I find a tendency to perceive the streets as having the more usual north-south and east-west directions. This tendency—which never quite prevails—is especially strong whenever I am in a house looking out of a window which offers a somewhat limited view. My knowledge before seeing the city of the irregularity of the streets, and the fact that my illusion when I came to the city was approximately 90 degrees, likely account for my not seeing the streets as consistently following the cardinal points of the compass.

Having left Omaha for Minneapolis in the night, I found myself "turned around" in the train in the morning. Cloudy and rainy weather made it impossible to right myself by the position of the sun, so the illusion persisted until we arrived in Minneapolis. The sun did not become visible for several days. In the meantime I had become acquainted in a general way with two sections of the city, the part of south Minneapolis between Tenth Street and the river, and Hennepin Avenue and Sixth Avenue South; and the part of southeast Minneapolis between the river and Eighth Street, and Central Avenue and Fourteenth Avenue, including the old campus of the University of Minnesota.

Three factors, then, evidently contribute to this illusion of direction: (1) The relaxation while asleep in a moving train, where bod-

[4] I recently witnessed the restoration by hypnotism of the personal memories of an individual who had completely lost his past experiences. An injury to the head in an accident was the cause of the disturbances. The person did not even know his name and could not recall his vocation or line of work. The change to a "self" with a good memory of numerous experiences of the past—which we found later, however, not to be the true self—and the return back to the "accident self" were immediately followed by peculiar movements and facial expressions of surprise, seeming to suggest an inner feeling of readjustment.

ily attitudes could have little chance of functioning in the maintenance of the sense of orientation even if one were on the alert;[5] (2) the formation of rather permanent associations of parts of the city with this erroneous direction attitude before it could be corrected by noting the position of the sun; (3) the unusual direction of the streets in the sections of the city with which these associations were formed. I had become so accustomed to streets following the cardinal points of direction that this one condition had become a fruitful means of orientation when the sun was not visible. One does not usually get the concurrence of so many factors favoring the illusion. When the sun did appear it was not a sufficiently forceful check—as was the sight of numerous familiar objects in the Chicago illusion—to correct the false system of associations. Moreover, these associations were in this case already more numerous and permanent than were those in the Chicago case. Of the other members of the family, R, an adult, experienced no, or but very slight, illusions of direction; K, a boy of eleven years, was "turned" about the same number of degrees, and in the same direction, as myself; while two small children, ages five and eight—the latter slightly retarded in development because of sickness early in life—seemed to orient themselves wholly on the domi-centric basis about the new home. Questions asked these two children as to the cardinal points of direction —such as "Which direction is north?" or "Show me which direction is north"—were to them artificial and called out only varying guesses. The methods by which these two children learned the way to certain stores and to school also confirmed the belief that their orientation was domi-centric.

In other parts of the city where the streets follow the usual direction of the cardinal points I experienced no illusion, if my visits to them were made while the sun was visible. This seemed true whether I walked or went in a street-car or in an automobile. Could the illusion, therefore, not be permanently dispelled if I noted carefully my directions along the way on my return? On several occasions I tested this logically plausible supposition. Regularly the illusion disappeared when I got outside of the sections visited on my first (cloudy) days in the city. Usually the disappearance was so gradual that its exact time and place could not be noted. At certain places in the borders of these sections there was much uncertainty experienced as to just what really did seem north! Sometimes on returning I approached by some particularly new way so near the region of the illusion that I fully expected to see it forced

[5] The importance of a general bodily orientation has been experimentally demonstrated by E. Mach, "On Sensation of Orientation," *Monist*, 1897, though he does not apply the experiment to direction orientation.

away completely, as was done in the Chicago case just given. At one time, returning from the state fair grounds on the street-car, I had come so near to the area of the illusion with the correct sense of direction preserved that the car had to make but a single turn—a distance of three to six rods. I remember saying to myself: "Now there is a surprise in store for me." I expected to see the university grounds, which were near at hand, in their true position with respect to direction. But suddenly as the car turned the whole "ground" gained seemed to slip—and the old erroneous view returned. No vertigo was experienced, but there was a decided feeling of discouragement: the situation seemed too big to be controlled without some sort of bird's-eye view of the surroundings. At this time, and also at the times of the following experiments, I had with me a small pocket compass which I had purchased with the view of correcting the illusion.

An example of the gradual change is interesting. On August 26, 1915, I walked to Seventh Street on Hennepin Avenue. By the time this point was reached my sense of direction was shown by the compass and by the position of the sun to be approximately correct. I began my return from this point at about 2:30 P.M., the exact time not being noted. On the return I walked toward the northeast, and the shadow was consequently cast nearly straight ahead, *i. e.*, in the direction I was walking. Before crossing the river I turned slightly to the right, or slightly more to the east than I had been walking. Some distance beyond the river, at Fifth Street on Central Avenue, the street turns approximately 30 degrees nearer toward the north, or to my left, taking a direction somewhat north of northeast. Each of these turns confused me slightly, so that I could not determine exactly just what did seem to be north. Occasionally I felt that my sense of direction was about right; again, when I tried to think of the relation of the street I was on to southeast Minneapolis, it would seem to be considerably wrong, my north being placed too far toward the east. I expected, however, to make a considerable correction on my illusion as I could approach the "dangerous" section gradually and constantly check up by the position of the sun and by the compass. At Seventh Street on Central Avenue, which I was following, there is a sudden turn of about 90 degrees to the east, to my right, giving me a direction somewhat east of southeast. This was the critical point. If this turn could be made successfully I should be victorious. All that I had gained, or held up to this point, slipped away gradually, however, as I saw the familiar scenes from Fifth to Seventh Streets. At such times one has a peculiar feeling of helplessness. Before reaching the point of this change I could imagine myself at some elevated point seeing the streets lead off from Central Avenue at 90

degree angles, and could consistently image them as following a southwesterly direction. But to proceed I felt was to lose all that had been gained.

Under conditions of such illusions of direction it is incredibly inconvenient to think of the larger divisions of a city and to try to get a consistent view of the whole situation. This is especially noticeable if a stranger asks you for directions to a place in the city outside the area in which you orient yourself in the domi-centric manner. It is the demands of a general social consistency that makes an erroneous attitude or perception so distressing and its correction so urgent. In certain parts of a city one can soon build up a workable domi-centric system for orientation; but when conformity to the activities and conceptions of others is demanded, or when one goes to the map for general directions to more distant points, then trouble arises. While the map is useful in studying the general features of the city, giving relations of one part to another, one finds it necessary to turn it according to one's own direction sense if one is going to some remote part of the city. While the map shows the university to be southeast of my residence, I continue to find it necessary to walk toward the northeast to get it! But suppose that I want to go to some place due east or north of the university, then I am ridiculously confused.

These experiences bring to mind the need of careful attention in the child's geography lessons to the development of the consciousness of direction. I have found that because of the irregularity in the plan of the streets of Minneapolis many of its citizens of long terms of residence have two systems of direction, one that is *known* to be the true one and one that *appears* to be the correct one. This has been found also to be the case in other such cities. Certainly such a condition is sufficiently important to demand the careful attention of public-school teachers. In the arrangement of rooms and of other means for the teaching of geography certain general principles in the development of the sense of direction should be kept in mind. Great aid may also be derived by having certain students carry a small pocket compass which can be had, with the necessary accuracy for practical purposes, for a nominal price.

There are evidently a number of similarities between the development and maintenance of the sense of direction and the development and continuity of personality. Both are dependent upon a multiplicity of factors all of which tend in either case to build up and preserve a practically permanent and comprehensive attitude. The absolute permanency of the self, maintained by the older philosophers, is likely a fiction: the self is gradually changing. Its memories, its likes and dislikes, its acquaintances, its intellectual tastes, its ideals, and its very self-thought are all changing as truly as anything in our

world of living forms. But there is a general continuity throughout these changes. The high degree of constancy of our surrounding physical stimuli, of our own physical appearance, of the names by which we are known, of the attitude of friends toward us, of various social institutions, of spoken and printed language—all these stimuli, and many more, are constantly tending to maintain for us one general attitude, one personality. This personality has, however, many variations, and may by unusual circumstances of disease or accident suffer profound disturbances. If under such circumstances a new personality emerges, it is necessary for the prevention of its permanency to see that the external conditions, social and physical, be made constantly to change as much as possible so as to prevent the establishment of lasting associations.[6]

So we find it also in the case of the changed sense of direction. The first few moments are crucial. Almost any one has likely been momentarily "turned around" in some new place and then has been set right by certain familiar marks, as the sun, the north star, or the sight of some distant mountain. As soon as a person finds that he is "turned around" even to a small extent, it is necessary that he at once take steps to correct the erroneous perception. There are times when this is for the moment impracticable. If at such times the person is on a moving train, or amid surroudings which are constantly changing, there is no need of any anxiety: the associations under such conditions can not become permanent. If, however, the erroneous attitude is seen to persist rather consistently even amid the constantly changing conditions, then it is highly probable that any cessation of these conditions will leave the person "turned around." Such consistency in the illusion is most likely to develop during a night's sleep on the train. On leaving the train and going into a strange city it is worth while to bend every effort toward dispelling the illusion. In this undertaking the following suggestions may be of value: If the sun is not visible the true north should be determined as accurately as possible. For this purpose it is a good thing to have, when traveling, a pocket compass. Other means are available, however. If the direction ascertained to be the true north does not appear to be north, the person should avoid as much as possible going out into the city. On going to one's hotel it would be well to take as little note of general directions as possible, so that erroneous associations will not be too firmly established. Once inside the building, however, where some time is to be spent, it would seem advisable to ascertain the true north and to make every effort so to perceive it. When the sun becomes visible the exploration of the city should begin from some point where the streets follow the cardinal points of the

[6] See Sidis and Goodhart, "Multiple Personality."

compass, or from which a general outline of the city can be obtained. The general rule would be to get acquainted with a strange place only when the usual stimuli by which we determine directions in practical life are most obvious and helpful, and to look about as little as possible at other times in case there is a tendency to illusions of direction. Everything depends on the early experiences in a new place, when associations which become basic are being established.

From the foregoing observations it would seem that one is justified in maintaining that in the waking state of consciousness a person is in a condition of tension of adjustment not only with respect to the more or less isolated objects of observation, but also with respect to their interrelations on a very extensive scale; that experiences are not each of a somewhat momentary and isolated character, but that they overlap in such a way as to call out comprehensive motor attitudes so that the effect of each stimulus is carried over in a cumulative manner not to be explained in terms of single neural pathways; that this cumulative motor attitude constantly comes into conflict with other such attitudes, bringing about certain confusions in orientation which may persist through life if their opposition is not reduced in the early stages of the association processes; and that these attitudes are kept up by the constantly recurring conditions of stimulation afforded by certain unchanging or slowly changing aspects of the physical and social environment. These motor attitudes[7] doubtless relax during sleep, but are again resumed on waking because of numerous direct and indirect stimulations similar to those of the previous waking state.

The writer has suggested in another article now in press,[8] that on the basis of this overlapping of the effects of individual stimuli we may explain the survival of the pleasant or successful acts in the process of learning over those that do not bring pleasure or success. Acts are not individual or discrete things, some of which survive as such while others are wholly eliminated. It is difficult to understand how the pleasurable "act" survives while the others are dropped off, unless one is willing to subscribe to the view that pleasure of itself

[7] Recently a number of psychologists have given more or less explicit attention to "attitudes," but usually of a more limited kind than those here studied. There is yet no general agreement as to whether such attitudes are ultimately analyzable into sensory elements or whether they are at times wholly motor and "imageless." Some writers have called them "conscious attitudes," while others, regarding them doubtless also as conscious, have used the term "motor attitudes." All writers seem to be at an agreement in regarding attitudes as being characterized by tendencies to response, whether as in "perceptual responses" or as in more overt activity. Of late it has been made plain that many acquired dispositions to certain types of responses are wholly unconscious.

[8] To appear in the *Psychological Review.*

"stamps in" the act which produces it. The difficulty lies in not understanding how an act can be emphasized by a successful result which follows it and which must, therefore, have a backward influence upon it. If responses overlap, forming larger unities, is it, then, not conceivable that no "act" is complete, in the higher organisms at least, until all responses of its own complex—*e. g.*, those directed toward getting out of a problem box—find expression together? May there not be mutual inhibition and delayed response until some consistent outlet involving mutual reinforcement is found by the trial and error processes? It is hoped that this hypothesis will tend to put the explanation of learning on a more objective and experimental basis than the conventional one of mere neural processes, often vaguely conceived or wholly hypothetical.

JOSEPH PETERSON.

UNIVERSITY OF MINNESOTA.

THEORY AS TRUTH: A STUDY OF THE LOGICAL STATUS OF SCIENTIFIC THEORY[1]

"Although in this long journey we miss the intended end; yet are there many things of truth disclosed by the way; and the collaterall verity may unto reasonable speculations some what requite the capital indiscovery."[2]

THE greatest feats of science to-day—for science is the acquisition and communication of knowledge—are, I suppose, not its facts, much less its deeds, but in very truth its theories. The theories of modern science deal commonly with things that we can never hope to reach with our senses, with matters that are beyond the reach of immediate experience. To questions which have been under discussion through the ages, questions such as those of the atomic constitution of matter and the nature of light, science to-day in its theories gives perfectly definite answers. The nature of the case forbids that these questions shall be answered by facts. When the theories which answer them, however, are submitted to the scrutiny of logic, it appears that they are not hypotheses in any sense and that they have a certainty of the same order as that of *any bit of experience*. It is as certain that matter consists of atoms as it is certain that experiments can be performed in the laboratory—and the opinion that matter is made up of atoms is no guess: we may be as sure of it as we are sure that there is matter at all.

[1] The notions here developed were first worked out for Professor Royce's seminary, 1914–15.

[2] Sir Thomas Browne, "Pseudodoxia Epidemica," Book VI., Ch. 12.

Unfortunately, the scientists themselves use the word, *theory,* loosely and without any exact conception of its application. One says: "A theory makes it possible to understand and remember an extraordinary number of facts with little effort. A theory is a short way of describing a group of phenomena."[3] "A theory," says another, "is a supposition we hope to be true; an hypothesis is a supposition which we expect to be useful."[4] Still another believes a theory to be an especially plausible kind of interpretation.[5] It seems unnecessary to multiply instances. The loose use of the word is evident.

Law has been defined for science, and the inductive method has been studied very thoroughly by logicians, notably by Charles Peirce. The use of hypothesis as a stimulator of research and a maker of *law* has been described; and hypothesis has been exalted to the highest by pragmatism. Theories, so far as I know, have not been defined. The need of a definition is all the more crying inasmuch as there are some "theories" in science which are neither laws nor hypotheses. Some of the so-called "theories" are actually hypotheses, some of the so-called "hypotheses" belong to this third class of knowledge and, although *workable,* they possess a certainty wholly beyond that inherent in their workableness. Hypothesis is *possibly* true, law is *probably* true, and *theory* is as true as a fact. "What?" said Dr. Johnson, in talking about the metaphysics of Berkeley, "to tell me that *that,*" here he kicked the table-leg, "doesn't exist"? Whatever may be the merits of the arguments for the existence of *that,* if you grant that it exists at all, then you grant at the same time that it consists of atoms—or, if we only dream *that,* then, in a world where there are such dreams, there is only one correct way to dream—which is that *that* consists of separable particles.

In this paper, therefore, I shall attempt to point out that there exists, among the mass of knowledge which constitutes science, a certain class of opinions which are logically different from laws and hypotheses. Many of them are already called "theories." For all members of the class I propose the name *theory.* I shall show that theories possess a special kind of certainty. They are the surest thing that science knows.

I. Instances of Theory and of its Scope

In 1849 Fizeau[6] performed an experiment in which a beam of light was made to pass between the teeth of a toothed wheel to a dis-

[3] Daniel Comstock.

[4] G. J. Stoney.

[5] Theodore Richards.

[6] Fizeau's experiment was not the first consideration to indicate the space-time quality of light. I have selected it because it seems clear and suitable for the present purpose.

tant mirror from which it was reflected back again over the same path to the eye of an observer. As the wheel was rotated, a beam of intermittent light passed through to the eye. When the speed of the wheel was increased, a point was reached at which the light was totally eclipsed and none passed through to the eye. That is, during the time required for the light to pass between the teeth to the mirror and back again to the place through which it came, during that time the wheel had moved so that the point in space occupied by the opening between the teeth was now occupied by the tooth itself, and no light could get through. At twice the speed of rotation required for this first eclipse the light again passed through to the eye. The light passed through one notch in the periphery of the wheel to the mirror, whence it was reflected back to the wheel, now to pass through the notch next the one through which it came. At three times the original speed the second eclipse occurred, and so on.

Although in describing this experiment I have been forced to speak of the time required for light to travel from one place to another, it is still evident that the experiment might be observed without any foreknowledge (or after-knowledge, for that matter) of the velocity of light. All that is seen is an arrangement of mirrors and lenses, a rotating toothed-wheel, and sometimes light through the eye-piece, sometimes not, according to the rate of rotation of the wheel. But, *unless light is something which has a velocity,* these things could not be observed. That is the theory. It is *true,* as the older physics books used to say, that light is a "mode of motion."[7]

Theories, when discovered, usually have the effect of destroying some hypothesis which was held earlier. Descartes guessed that light was "the effect of a pressure instantaneously transmitted through a universal medium."[8] The fact that light has a velocity means that light can not be the effect of a static pressure. When it became clear that light was a mode of motion, two rival hypotheses sprang up—the emission or "corpuscular" hypothesis of Newton and the undulatory hypothesis of Huyghens. Both have always been called "theories," but both were pure guesses, made because they offered what seemed to be plausible explanations of certain phenomena. There was a little

[7] Unfortunately theories are usually expressed as universal statements. The real theory, which is true, is that light (*the particular light which was used in the experiment*) has a velocity. The statement that light (*all light*) has a velocity is something else—law or convenient definition, I do not know—and, of course, the statements here made about the truth in theories do not apply to that universal statement. In the following discussion I shall express certain theories in the form of universal statements, for such is the habit of science. But real theories are, in strict logic, particular statements—as particular as the experiment.

[8] Duff, "A Text-book of Physics," page 360.

truth: these, in addition, supplied gratuitous opinion. I know no better name for them than the one used by William James in discussing religious experience. They were "over-beliefs."

The hypotheses of Newton and of Huyghens seemed to explain about equally well the phenomena of reflection and of refraction. The discovery of interference eliminated one of them. Some thinkers in the methodology of science have called the experiment of Thomas Young on interference a "crucial experiment"—because it decided between two rival hypotheses. It is that, but it seems to me that there is something about it far more important. *Even if there had been no Newton and no Huyghens, the experiment of Young,* properly interpreted, *would have led to* a certain theory, a certain truth—*the undulatory theory of light.*

In Young's experiment, light from a single source is made to pass through "two narrow slits, very close together, which act as secondary sources. If a screen be placed beyond these slits a series of colored and dark bands parallel to the slits will be observed on it. If one of the slits is covered, the bands disappear. This shows that they are the resultant effect of two superimposed pencils of light alternately reenforcing and destructively interfering with each other."[9] Unless light is a wave motion, there could be no reenforcement and interference, and the screen, placed beyond the slits, would be evenly illuminated in all its parts. Given this observation, it becomes certain that light is a wave motion. That is the theory.

It is to be noticed that a specific fact, or law, or set of laws, renders certain such and such a truth—but there is still room for over-beliefs. Another law is discovered which renders certain a little more of the truth and does away with part of the over-belief. When enough laws have been accumulated the situation becomes closed—within this small area we have truth—within these limits there is nothing more to be said. Light is a mode of motion—the area is mapped out. Light is a wave motion—there is no more truth obtainable within this area. The result of this is that the theory is necessarily broad in its phraseology. It does not go into details. That which does go into details and which may be expressed exactly is the law—which is probable.

Boyle's law states that, the temperature being constant, the volume of a gas varies inversely as the pressure. Charles's law states that, volume being constant, the pressure of a confined volume of gas varies directly as the absolute temperature, or, that, pressure being constant, the volume varies directly as the absolute temperature. The two laws may be combined, thus: $PV = KT$. It is probable that the pressure-volume product for a confined quantity of any gas is directly

[9] Duff, *loc. cit.*, page 364.

proportional to the absolute temperature. *Unless* gases are alike in their inward constitution this could not be. It is true then that gases are *mechanically* alike. That is the beginning of a gas theory.

Gay-Lussac's law says that gases combine chemically with one another volume for volume or in proportions by volume which are expressible as the ratios between small whole numbers. Thus, one volume of hydrogen combines with one volume of chlorine to form hydrochloric acid, one of oxygen combines with two of hydrogen to form water. There is also the law that elements combine with one another in definite ratios by weight. On the strength of this second law a *chemical unit* (an atom) may be defined as that quantity by weight of a given element which will combine with a unit of some standard element—with a unit of hydrogen. Now, from the first law, which is probably true, it is possible to get a theory which is definitely true: for, *unless* equal volumes of gases contain numbers of chemical units which are relative to one another as the ratio between small whole numbers, gases could not combine in simple ratios by volume. Combine this now with the rudimentary gas-theory of the preceding paragraph and a very important theory results—the beginning of the kinetic theory of gases. Equal volumes of gases, under the same conditions of temperature and pressure, contain an equal number of *physical units*—the mechanical, pressure-volume things (molecules), and, if the gases are elementary, the molecules contain each a simple whole number of chemical units or atoms. This has been called "Avogadro's Hypothesis" after the man who first enunciated it. It is of the utmost importance to physical chemistry. But it is not an hypothesis, it is not a guess; it is a theory—and true.

With this bit of theory available, we are in a position where it is relatively easy to learn more new truth. The question might arise: How many atoms does the hydrogen molecule contain?—or, in another form: Is the chemical unit of hydrogen identical with the physical unit? One volume of hydrogen combines with one volume of chlorine to form two volumes of hydrochloric acid gas. Then the physical unit of hydrochloric acid contains two chemical units, one of hydrogen and one of chlorine. Let n equal the number of physical units contained in a volume of gas under the conditions of the experiment. Then we have that n physical units of hydrogen combine with n physical units of chlorine to form $2n$ physical units of hydrochloric acid. But the $2n$ molecules of hydrochloric acid contain $4n$ atoms. Then the n molecules of hydrogen plus the n molecules of chlorine must have contained $4n$ atoms. Moreover, the number of hydrogen atoms and of chlorine atoms was equal. Therefore, the hydrogen molecule and the chlorine molecule each contains two atoms. *Unless* each contains two atoms, one volume of one could not combine

with one volume of the other to form *two* volumes of a gas whose molecule, from the fact that it is a compound, is known to contain two atoms.

This bit of information, that the hydrogen molecule contains two atoms, is logically of the nature of a theory. So far as I know it has not been called either a law, or a theory, or an hypothesis. It seems less important and less universally interesting than the truth that light is a wave motion. Still it deserves to be pointed out that science is full of these little bits of knowledge which are not merely probable (as far more important laws, like the law of gravitation, are probable), but are true.

In a world where one kind of matter combines with another kind of matter in a definite proportion by weight, where gases interact chemically in simple ratios by volume, where the law that the pressure-volume product for a given quantity of gas is proportional to the absolute temperature is a genuine law, there are chemical units and physical units, or *atoms* and *molecules,* and equal volumes of gases contain an equal number of molecules. And the molecules are themselves made up of a perfectly definite and determinable number of atoms—where the atoms are still taken as chemical units in the same manner that the gram is a unit of mass. But in this world the question can not be answered whether or not atoms and molecules are truly particles—discrete and separable.

The fact that a piece of sugar dissolves in water proves nothing as to the inner constitution of the sugar. It is conceivable that the water merely increases the distribution of the sugar—that it is a medium through which the sugar attenuates itself. Sugar might well dissolve in water without either of them—for all we know—being made up of ultimate particles. The fact that a given quantity of water will hold in solution in stable equilibrium only so much sugar and no more—taken by itself—proves nothing. Iron and sulphur combine in definite proportions when heated together to form a new substance whose properties are different from those of iron or of sulphur, and whose properties are not, in any sense, intermediate between, or a blend of, the properties of the elements from which it is made. But it is not unreasonable to suppose that the iron dissolves the sulphur—that they distribute themselves one through the other—and there seems no reason, *a priori,* why the resulting solution could not have properties wholly different from those of the substances from which it is made. The law that elements combine does not require the theory of the atomic particle. Neither does the law of definite combining weights—for there seems no contradiction in supposing that a given quantity of a solvent will dissolve just so much, no more and no less, of another given substance:

if too much of the substance is taken there will be some left over, undissolved; if too much of the solvent, then there will be some of the solvent that is doing the actual dissolving and some that is left over, inactive. The law of multiple proportions, likewise, does not lead to the theory of individual atoms.

Chemists have long held to the notion that matter is composed of individual particles. For a long time the notion was an hypothesis —an hypothesis suggested by facts such as those just mentioned— but not required by them. It made possible clear mental pictures and neat descriptions of phenomena. It had the value of its pragmatic workings, but it did not possess the truth which attaches to a theory.

It does not matter whether the milkman adds the water to the milk or adds the milk to the water; the result in either case is the same. And in making a pudding the order in which the various ingredients are added is immaterial; if the same quantity of each ingredient is used each time, the same pudding will always result. Thus, if matter does not consist of particles, the order in which several elements combine to form a complex compound does not matter, and whenever the same quantities of the same elements combine the same compound will certainly result.

On the contrary, if compounds are built up of particles of the constituent elements, in a manner similar to that in which children build towers and houses from wooden blocks, then the arrangement of the particles is an important matter: and, just as different arrangements of the same blocks will give different sorts of houses or towers, so different arrangements of the same atoms will give different compounds. That is, it is impossible that there should be more than one compound of a given molecular weight and empirical constitution, more than one compound whose physical unit (or molecule) contains definite numbers of specific kinds of chemical units (or atoms) *unless* matter consists of particles. If two *different* compounds are known of the same molecular weight and constitution, then there is the corresponding theory—which is as true as the fact that the two compounds exist—that a compound is built up of particles and that the molecule itself is a particle made up of particles.

In 1823 Liebig demonstrated that silver fulminate and silver cyanate have the same molecular weight and empirical constitution. Such substances are said to be *isomers*, or *isomeric* with one another. Silver fulminate is a white crystalline solid, soluble in water, and exceedingly explosive. Silver cyanate is white, not soluble in water, not explosive, and is decomposed by strong acids. The discovery

that this harmless substance possesses the same composition as explosive silver fulminate created a great impression. In 1828 Wöhler changed ammonium cyanate into urea—and proved incidentally that the two substances are isomeric. Ammonium cyanate had been prepared before only from inorganic sources and urea only from organic ones. Ammonium cyanate is decomposed by alkalis to give the alkali isocyanate and one equivalent of ammonia. Urea is decomposed by acids and by alkalis to give two equivalents of ammonia and one of carbon dioxide. Wöhler's preparation of a typical organic substance from an inorganic one served to break down the artificial barrier between organic and inorganic chemistry. But, with the discovery of isomeric substances, the notion of the atomic particle ceased to be merely an hypothesis and became a theory—it entered into the state where it could be seen by reflection to be true. *Unless* matter consists of particles, there can be no isomeric substances.

II. Induction Gives Us Warrant to Theorize

With these instances of theory before the mind, it will be well to consider briefly the nature of law and of hypothesis. This will not only make the situation clearer by showing us opinions in science which are not theories; but, more important, it will show us by what right theories can claim to be true.

When I have seen a swan and seen that it is white, when I have seen no swans that are not white, when I see more white swans, then I make for myself the generalization, "All swans are white." This is the inductive method. The opinion that all swans are white is a law. It is *probably* true. It is *true* that it is probable that all swans are white. Mr. Charles Peirce has shown that *true* here possesses the same meaning as it does when we say, "It is true that a given thing can not be both A and not $-A$." That is, the probability is not merely one that satisfies our mind; it is a genuine probability—provided always that the swans, upon the observation of which I base my generalization, constitute a fair sample of all the swans. Thus, a law in science, like the law that an alloy of two metals has a lower melting-point than either of the metals taken singly, is as genuinely probable as the theorems of the algebra of logic are genuinely true.[10]

The scientist makes first an observation, and, as the result of it, an hypothesis comes into his mind—"Perhaps all the things of this sort are the same"—then he goes about to make more observations

[10] A discussion of Mr. Charles Peirce's contribution to the logic of induction may be found in Professor Royce's article in the "Encyclopedia of the Philosophical Sciences," Vol. I., pages 82 *et seq.*

to verify his hypothesis; finally, when he has found a number of instances in accord with his hypothesis and none in disagreement with it, his hypothesis becomes a law. That which was at first a pure guess is now probably true. Hypotheses are used in science mainly as a means of getting at laws, but they are not all so used. There are hypotheses that are nothing but fanciful guesses: their only excuse for being is that they give anchorage to ideas—and, alas, many of them go at present by the name of theories.

Professor Royce has pointed out that a *fair sample* is an instance which is not antecedently likely to be in conformity with the hypothesis which is being tested.[11] Mr. Bertrand Russell holds that the simple enumeration of instances—from whatever source, and without any consideration of the antecedent likelihood of their conformity—is sufficient to establish an inductive generalization.[12] He says: "The greater the number of cases in which a thing of the sort *A* has been found associated with a thing of the sort *B*, the more probable it is (if no cases of failure of association are known) that *A* is always associated with *B*. . . . For example, a man who has seen a great many white swans might argue, by our principle, that on the data it was *probable* that all swans were white." "Suppose," he was asked, "that a man has a pair of white swans, and that he has never seen any other swans, and that he uses this pair for breeding; suppose that, as his flock increases, he observes that each of his swans is white; is he correct in generalizing that all swans are probably white?" "Yes, he is," said Mr. Russell. This seems absurd, and I am sure that no scientist who has worked much in the field or in the laboratory would think of making such a generalization. He might say: "It seems probable that color, with swans, is an hereditary characteristic," but surely he would not say, "It is probable that *all* swans are white." Such cases as that tend to persuade one away from the views of Mr. Russell, but there remains still an *argument* against his opinions.

In arguing for the mere enumeration of conforming instances as against the enumeration of instances which do conform, but of which

[11] There is another way of deriving laws—by the discussion of the standardized experiment, but it seems to me that the experiment is itself *probably* a fair sample of all possibly suitable experiments. The experiment is itself a law. Also I believe that the so-called "facts" of experience are not what they seem to be. They also are the outcome of earlier inductions. The only real facts are the infrangible fragments of data which epistemology starts with. Fortunately, to the little inductions which are epistemologically earliest there correspond primitive theories. I hope to develop this matter in another paper.

[12] Russell, "The Problems of Philosophy," Chapter VI., pages 104–05.

none are antecedently likely to conform, he has said, in effect, "You suppose it probable that a certain instance is likely to be in conformity with your hypothesis. Well, this probability itself is the result of the simple enumeration of instances." "But," one might insist, "not at all, this probability has itself been established by an examination of instances which were not antecedently likely to conform." "Very well," Professor Russell may say, "carry your regress back as far as you like, carry it back even to the beginnings of experience where sense-data are accepted in all naïveté, you come at last to a position where you have no longer means of forming judgments about antecedent likelihood—you are *forced* to rely on the enumerations of instances." Now, I answer, in order that instances may be enumerated for purposes of induction it is necessary that they be in the same class—they must be identical in *this one respect.* On the borderland of epistemology, experiences are supposed which are atomic in their simplicity—they have only one property—they have only one mark by which they may be known. Given two such experiences, they either are identical or they are not. If they are identical, they will not serve as instances to illustrate a more general law—they are useless for purposes of enumeration. If they are not identical, then they are not antecedently likely to conform to any namable law. Clearly, induction depends upon the enumeration of instances, but of instances whose conformity is not to be foreseen.

Professor Russell says also "a sufficient number of cases of the association of A with B will make it nearly certain that A is always associated with B, and will make this general law approach certainty without limit."[13] Evidently he means that as the number of observed instances increases so the feeling of probability grows and grows until at last it amounts to a practical satisfying certainty. That is psychology, not logic. The interesting thing about certainty in induction is that the law, reached by an examination of *fair samples,* is *certainly* a genuine statement of what is probably true. For, "if we choose partial collections from a larger collection, and judge of the constitution of the whole collection from that of the parts chosen, fixing our attention upon definable characters present or absent, in the partial collections, we are aided toward probable inferences by the fact that there are *more* possible 'samples,' or partial collections, that at least approximately agree in their constitution with the constitution of the whole, than there are samples that widely disagree."[14] It is not merely probable that all A's are B's,

[13] Russell, *loc. cit.,* page 104.

[14] Royce (discussing Peirce), *loc. cit.,* page 86.

where the *probable* is a psychological thing open to discussion; if the law is properly made, *it is true that it is probable.*[15]

III. Theory as True as Fact

A theory in science is a statement of the conditions without which a certain law, or set of laws, could not be even possibly true. Whatever may be the meaning of "probable," surely it is something better than "possible." Suppose, for the sake of exactness, that the law states that, relative to a certain group of phenomena, the group itself being exactly defined on empirical grounds, A is probably the truth. We may then make a proposition, ϕ_1: A is possible, and another, ϕ_2: A is impossible. The difference between them, $\phi_1 - \phi_2 =$ *Conditiones sine qua non.* Let us call these conditions the "Body of Opinion, T." On purely deductive grounds one may say: "Unless such and such were the case A would not be possible; there are these *conditions without which* A could not be." In science the body of opinion, T, embodying these conditions is frequently sought after with eagerness, and, when found, it often comes to be known by the name of a theory. I assert that such bodies of opinion exist among the materials of science. They are often called *theories,* but not always. Nor do all so-called *theories* comply with the conditions here defined—many of them are actually pure guesses. It seems certain that the things which science makes, the end result of all its work, are of three kinds—*laws, pure guesses,* and the sort of bodies of opinion here defined. I propose to call the pure guesses "hypotheses," and the sort of things like the body of opinion, T, by the name of "theories."

If the law is true, if it is certainly true that A is probable, it seems to me to be equally certainly and genuinely true that the facts embodied in T are true also. The truth of the theory follows from the genuineness of the law.

In order that there may be a law, there must be at least one event in accord with it, and wherever there is a single event, even though that event gives rise to no law, there is still the set of conditions without which that event could not be. Single events, experiments, observations, as well as laws, give rise to theories. And if a philosopher is so skeptical as to insist that it is only probable that the observed event exists, so long as he admits that it is a genuine probability, then the facts embodied in the corresponding theory are

[15] The probabilities of the insurance underwriter are not genuine probabilities—they are only probably probable. They are derived from the examination of what are *probably* "fair samples." So it is in general with statistical probability. Fortunately, experimental science deals very little with statistical probabilities. A further analysis of statistical probability is needed.

still true. The man of science, I suppose, will not be disposed to question the existence of the event observed in his laboratory; and, for my own part, it seems to me that the presuppositions which common sense makes in regard to the material world are legitimate theories resulting from simple laws which now lie hidden, for the most part, beneath the flux of our unreflecting complex experience. But that by way of suggestion; it is another story and must be developed at another time.

The science of chemistry is peculiarly rich in theories. Tautomerism and labile bonds, stereochemistry, thermochemistry, and the relative stability of allotropic modifications, will show, I suspect, some interesting things when subjected to a logical study. It needs to be known whether the kinetic "theory" of gases and the ionization theory are true theories or plausible hypotheses. The recent work of the Bragges and of Irving Langmuir will probably lead to important *theories* concerning atoms and the structure of molecules. For the present paper it is enough if I have shown that the chemists' atomic theory (although in the early days of the science it was an hypothesis or overbelief) possesses, in the main, the same sort of truth as any bit of experience. Surely its certainty to-day is beyond the starry dreams of Democritus and Epicurus.

The theories of physics seem to be fewer and less vast than those of chemistry. Perhaps this is because, while chemistry is built upon a theory, the foundations of physics are the presuppositions of common-sense activity. Whether the other sciences contain many theories is important matter for research. It would be interesting to learn how much certain truth there is in the "glacial theory," the "theory" of organic evolution, the nebular "hypothesis," or the hypothesis that "the moon came out of the Pacific Ocean."

Finally, it deserves to be noticed that the theorizing tendency, the tendency to seek deductively the conditions without which a certain phenomenon, or group of phenomena, could not be, is one which has characterized the great advances in scientific knowledge. The advent of a new theory has been the index of the advent of genius. Genius the most genuine has been needed to produce real theories. If, in the foregoing, I have chosen poor instances of theory, it is ascribable to ordinary frailty; but the logic remains—and the fact that the great men of science can, and do, tease out of phenomena this illuminating collateral verity.

TENNEY L. DAVIS.

SOMERVILLE, MASS.

REVIEWS AND ABSTRACTS OF LITERATURE

Sādhanā: The Realization of Life. RABINDRANATH TAGORE. New York: The Macmillan Company. 1913. Pp. 164.

Mr. Rabindranath Tagore, recipient of the Nobel prize for poetry, enjoys a popularity perhaps exceeding that of every other living native exponent of Asiatic thought to the West. The present book, which has now been published for some time, is his first in English prose unless we regard as such the unmetrical translations of his verse. As an interpretation of far Eastern ideas in terms palatable to the Westerner, and also for soft persuasiveness of style, it compares with the writings of Fielding Hall and Margaret Noble. For charm of thought, imagery, and words, it may be accepted as above criticism. Mr. Tagore was born and reared in an environment of Hinduism, he is a teacher in schools of his country, he has discerned it deeply, and he brings up from it much that is unlocalized and inspiring.

In developing his subject he considers first the relation of the individual to the universe, showing how the Indian feels his oneness with nature, but especially with the divine soul therein. This, further, is to be realized through consciousness of one's own spirit and of its powers for blessed fruition. Evil is disposed of as a condition necessary to the working out of good. Selfhood is emphasized with the strong Brahman sense of personality which, however, finds its true meaning not in separation, but in union with the All. Life may be realized through love, beauty, and action. The key to the universe is joy, which expresses itself through law, but transcends law, and for which love is another name. Realization through action, through the joy of work, is Western in spirit, and Mr. Tagore censures some of his countrymen for undue dreaminess, though he appeals for support to the Upanishads. One is reminded of Aristotle's saying that "pleasure perfects the act of working, and so life, after which men grasp," but Tagore carries the idea into ethereal regions unknown to the peripatetics. The final chapter, on realization of the infinite, suggests a class of literature not unfamiliar in occidental book-stalls.

Obviously this teaching is orthodox Brahman in substance, but Mr. Tagore has imported into it a poet's fervor, a modern man's rationalism, and an optimism distinctive of sunset lands.

As a religious eclectic, affiliatetd, it is understood, with the Bramho Samaj, he refers to Jesus and to Buddha sympathetically. The latter he frequently quotes as teaching doctrines in agreement with his Brahmanism and he tries to interpret away the Buddhist negations. If he has strained even Hinduism to accommodate his expansive optimism, he has burst the Dhamma asunder. True, he cites a discourse of Buddha with Sádhu Simba (Sīho of Vesāli, mentioned in the Vinaya) which, if it stood alone, would go far toward justifying him, but this passage is so exceptional in Buddhist apologetics that if it were reliable it would render much of the canon meaningless. Again, in writing of infinite love, Mr. Tagore says: "Buddha names it Brahma-vihāra, the joy of living in Brahma." This is

very misleading, since Buddha had no use for any Brahma in so mystical a sense.

An ambassador of the East to the West, we must look upon Mr. Tagore's office as mediatorial, and mediation implies compromise. In one sense this is just the work needed, a blending of extremes, a participation of benefits. But is Mr. Tagore free from that compromise which is to win favor by surrendering principles needed by us occidentals as a bitter corrective? Is not rather his popularity in America due to his expressing for us our pet enthusiasms in the swart paynims' romantic forms? If there is one quality paramount which India has proclaimed and of which the Western world to-day stands in dire want, it is *pity*. To this Gotama Buddha gave the clearest voice, but his feeling was not alien from that of his countrymen. To realize not merely life, but what life means to multitudes who suffer intensely, this the West hates as pessimism, and in glorifying the optimism which its own lot facilitates, it has blinded the eyes of its sympathy. *Ex oriente lux* we cry, and we welcome it so long as it is the light that dazzles rather than that which would enable us to see the hard facts of human and animal existence. Mr. Tagore is doubtless a tender-hearted man—his books prove it—but the lessons of his vivacious enthusiasms, not to mention the occasional voluptuous fervors of his poems, are far different from the chastened compassion that India might teach us. His complacent view of pain, with its disciplinary value, is unfeeling, like that of John Fiske, for example. Perhaps it is true that all suffering may be justified by its relation to the general plan, but in any aspect that concerns us practically a large part of the misery in the world is wholly without benefit. Let us gladly accept his chapter on fellowship with nature, but in a lowlier sense than his. Ours is no time for an apotheosis, but for a kenosis.

However, it is not assumed that many Americans will concur with this dissenting opinion, which in one way is a compliment to Mr. Tagore, since it holds him to account for the use he is making of a great influence.

EDWARD P. BUFFET.

JERSEY CITY, N. J.

Business Psychology. HUGO MÜNSTERBERG. Chicago: La Salle Extension University. Pp. 296.

This latest volume by Dr. Münsterberg is intended to furnish the man in business and industry a genuine scientific basis for dealing with the many problems involving the human mind with which he has to deal. Although the author states that the reader who expects to find light, snappy, gingery, and withal superficial treatment, will be disappointed, the fact remains that the book is written in language and style that make it readable by the ordinary layman, and at the same time lay a solid foundation. The first three chapters deal respectively with "Business and Psychology," "Scope and Methods of Psychology," and "The Application of Psychology." In these preliminary chapters the author demonstrates that there is a definite science of the human mind as solid, as scholarly, and as systematically worked out into principles and laws and methods as

the sciences of physics and chemistry. He emphasizes the fact that man is more important than machinery, thoughts more valuable than equipment, and personality the biggest factor in industry and business. He shows how the educator, the doctor, the lawyer, and the artist are applying the laws of psychology, and bespeaks a similar application to the wider fields of economic endeavors.

Chapters IV. to XI. deal with "Mind and Body," "Sensation," "Perceptions," "Memory and Ideas," "Attention," "Feeling and Emotion," "Impulse and Will," and "Suggestion." These chapters are stripped of all technical nomenclature except where technical terms are unavoidable and a knowledge of them is necessary. They are rich in practical examples. For instance, in explaining what a percept is he says: "We perceive a page in Japanese print exactly as well as the Japanese perceives it. The fact that he connects with every sign an idea which gives meaning to it lies outside of the process of perceptions."

Chapters XII. to XVIII. deal with application of principles, being entitled, respectively, "The Acquirement of Abilities," "The Outer Conditions of Efficiency," "The Inner Conditions of Efficiency," "Vocational Fitness," "Individual Mental Traits," "Selection of Fit Individuals," and "Mental Tests."

No attempt is made to use and define such terms as "Pre-perception" or "Apperception" and similar expressions which have made psychology a bugbear and a nightmare to school teachers. It takes a man like Münsterberg, who is an acknowledged master of the higher technology of his subject, to write in such a style. The author shows an understanding of the business man's and manufacturer's problems and an appreciation of the work and field of the engineer which the writer has not as yet found in any other work of the sort. Moreover, the reader gets no sham, but real psychology with a characteristic German thoroughness. Dr. Münsterberg has done a public service in writing such a book.

The publication of such a practical and thoroughly scientific text is indicative of the progressive spirit of the modern correspondence school. Competition of state universities and colleges and bureaus of extension education have led the private correspondence institutions to secure the highest authorities available as text-book writers, and in no case has an author comprehended the need and the opportunity more clearly than Dr. Münsterberg. Another indication of the progressive policy of the publishers is their willingness to sell this and other texts as separate books to the public at large at the reasonable price of $2 a volume without the necessity of buying a whole "set" of correspondence texts.

HUGO DIEMER.

PENNSYLVANIA STATE COLLEGE.

JOURNALS AND NEW BOOKS

THE PHILOSOPHICAL REVIEW. November, 1915. *The Determination of Human Ends* (pp. 583–602): A. K. ROGERS. – The selection of aims, ends, or social ideals is not made by an appeal to fact, to opportunism, or to custom, nor to science. If science could predict, then it might furnish a criterion, but variation means novelty, hence the future is unpredictable. Ideals are determined by ourselves, that is, they are subjective. *Was Plato an Ascetic?* (pp. 603–613): HERBERT L. STEWART. – Finds justification for the view expressed by Mr. R. W. Livingston in his recent book, "The Greek Genius and its Meaning to Us," that Plato is un-Hellenistic. Plato's predominant mood was Puritanic; "he struggled in vain to overcome what nature had made him." *The Philosophy of Pierre Charron* (pp. 614–630): NORMAN WILDE. – The modern reader of the *Sagesse* is impressed as much by its beliefs as by its doubts. The skepticism of Charron is "Superimposed on a dogmatism as vigorous as itself." This combination of skepticism and dogmatism is explained in terms of an "eclecticism dictated by his pragmatic bent." *Discussion: Pragmatism, Science, and Truth* (pp. 631–638): A. W. MOORE. – A critical reply to Professor Fite's articles on "Pragmatism and Science" and "Pragmatism and Truth." Needs, for Professor Fite, are both "spiritual" and "practical"; for the pragmatist they are all practical. Professor Fite complains that the pragmatist ignores the problem of the independence of truth; for the pragmatist there is no such problem. *Reviews of Books:* James Mark Baldwin, *Genetic Theory of Reality:* EDWARD L. SCHAUB. François Picavet, *Essais sur l'histoire générale et comparée des théologies et des philosophies médiévales:* HORACE C. LONGWELL. John Theodore Merz, *A History of European Thought in the Nineteenth Century, Vol. IV.:* J. E. CREIGHTON. Wincenty Lutoslawski, *Volonté et Liberté:* G. N. DOLSON. *Notices of New Books. Summaries of Articles. Notes.*

Lloyd, Alfred H. Incarnation: An Essay in Three Parts. Reprinted from the American Journal of Theology, Vol. XX, No. 1, Pp. 35.

NOTES AND NEWS

IN the *Messenger of Europe* (*Vestnik Evropi,* June 1915, Vol. 293, pp. 157–168) there appeared an article by Kovalevsky, entitled, "A Page from the History of our Relation with Western Philosophy." Such an article would be welcome even to the layman, now that Russia is engaged in the "War of the Nations," but to those who are interested in philosophy the article bears a deeper significance, for it enables one to look at Russian philosophy, not as an isolated system, but as having numerous connections with the philosophy of Western Europe. Kovalevsky begins with the influence of Schelling and Hegel on such men as Staukevitch, and the attention given by Hertzen, Bakounin, Souchovo-Kolilin, Proudon, Gra-

novsky, B. N. Chickerin, Jemchoujnikoo to German metaphysics, especially Hegelian. From that time on, Russian philosophy has always been in the harness of German metaphysical thought, for the interest in Hegel was succeeded by that in Feuerbach, then in Schopenhauer, and now the topic in the universities is neo-Kantianism, as is especially the case in the lectures of A. D. Uvedensky and I. I. Lopshin. The stream of scientific philosophy, represented by Comte, Spencer, Mill, Lewis, Bain did not interest the philosophers as much as the work of the natural scientists, jurists, historians, writers on descriptive and normative sciences and pedagogues. In this the greatest influence was exerted by the English and the least by the French. As the immediate leaders of the French positivism, opposed to the last attempt of Comte to construct a religio-metaphysical system by means of the subjective method, we may designate G. N. Viroubor and E. V. De Roberti, although their influence in the Romance countries was by far greater than in Russia. The former devoted most of his life to research in chemistry and crystallography, and to lecturing at the Collège de France. He also edited the *Journal of Positive Philosophy,* which began in 1867. But it was De Roberti who did more than any one else to introduce the positive philosophy into Russia and from it proceeded a new philosophical current known as neo-positivism. Kovalevsky devotes about three quarters of his paper to the works of De Roberti. We shall therefore mention a few facts here. In 1869 he published his "Politico-economical Studies," wherein he subjects the current doctrines of the economists to a criticism from the point of view on which Kant insisted in his "Social Statics." But in 1875, in an article in "Knowledge" (Znanie) he showed his difference of opinion from the positive philosophy and sociology of Kant. De Roberti himself regards this article as including the potentialities of his whole philosophy, neo-positivism. In 1880 De Roberti wrote his "Sociology," in which he divides Comte's six sciences of abstract knowledge into disciplines investigating (1) organic, (2) inorganic, and (3) super-organic phenomena. After about twenty-five years De Roberti published his "New Inquiry into the Basic Questions of Sociology," in which he develops his bio-sociological hypothesis concerning the nature of super organic phenomena, raises the question concerning the natural history of Society, as, also, concerning the foundations of abstract sociology. Here he distinguishes four factors of culture: (1) science, (2) philosophy and religion, (3) art, and (4) action and conduct. De Roberti also dealt a blow to agnosticism with his "Unknowable" (1889) and "Agnosticism" (1892). During the later part of his life he preached his philosophy in the psychoneurological institute at Petrograd. Kovalevsky expresses his regret that De Roberti, because of the Russian censure, was forced to publish most of his works in French, instead of Russian. As a result the man who did the most toward bringing the Russians into a closer relation with the philosophy of western Europe, is not sufficiently well known in Russia.